LETTERS TO THE EDITOR.

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Precursors of Magnetic Storms.

IN kindly noticing (NATURE, December 30, 1909, p. 259) my short account of the magnetic storm of September 25, attention was directed to the fact that what I called the precursor was experienced all over the world.

As I have little or no opportunities to compare our curves with others, may I be allowed to ask for information through the columns of NATURE?

I call a "sudden start" of the magnet a movement which occurs after several hours of perfect calm, and causes the trace to make a "sharp angle," that is, so sharp that I can unhesitatingly tell the time of the occurrence to the nearest two minutes. Many disturbances, large or moderate, have a "sudden start"—at least here.

My impression has long been that disturbances with a "sudden start" very generally have a kind of "preliminary tremor" some hours before the start; the curve, which we suppose to be quite smooth, is interrupted by a short movement, which lasts but a few minutes, after which the curve resumes its smoothness for the remaining hours. The tremor may be very small indeed, but the two characteristics, to be found on a smooth curve, and to be of very short duration, make it quite easy to point it out and tell the time.

The start of the "preliminary tremor" is in the same direction as that of the disturbance itself, at least as a rule.

I do not venture to hold an opinion as to the connection, fortuitous or otherwise, between the two phenomena, but I should be very glad to know whether the "precursor" is also observed in other countries. The following is the list of all the "sudden starts" of H during the last fourteen months of Zi-ka-wei (January, 1907, to February, 1908, inclusive), with the time of the start and that of the preliminary. A comparison with the curves of some other observatory is invited. Probably the traces will not be found so smooth as here, and some of my "sudden starts" will correspond to progressive starts, but on the whole I hope that a comparison will be possible.

I use Greenwich time.

	Sudden starts	h. m.	Preliminary	h. m.	Remarks
1.	Jan. 8	16 45	Jan. 8	1 45	
2.	,, 11	8 45	,, 10	3 10	H not quite smooth. D very smooth.
3.	,, 14	19 35	,, 14	10 30	
4.	Feb. 7	8 10	Feb. 6	23 55	
5.	,, 9	14 12			Curves not quite smooth.
6.	,, 14	4 37	,, 13	19 47	
7.	Mar. 10	5 3	Mar. 9	16 (?)	Curve not quite smooth.
8.	,, 11	17 23	,, 11	13 45	
9.	,, 21	13 33	,, 21	9 50	
10.	May 18	13 58	May 18	10 52	
11.	June 3	22 55			Curves not smooth.
12.	,, 18	3 42	June 17	17 20	
13.	July 10	14 23	July 10	11 0½	
14.	,, 25	4 18	,, 24	14 40	There is another preliminary at 3½.
15.	,, 28	0 12			Curves not smooth.
16.	Aug. 14	15 3	Aug. 14	11 0	
17.	,, 20	2 24	,, 19	13 30	
18.	,, 30	7 15	,, 30	5 8	And also 29 at 11h. 12m.
19.	Sep. 10	1 50	Sept. 9	15 0¼	
20.	Oct. 13	7 45	Oct. 13	0 0½	
21.	Nov. 21	10 45			The curve of H was lost, until 10h. But D does not show anything.
22.	Dec. 4 1908	5 45	Dec. 3	20 57	
23.	Jan. 27	13 48	Jan. 27	0 25	
24.	Feb. 22	12 8	Feb. 21	19 20	

If we put aside No. 21, we have twenty-three occurrences. In eighteen of these there is no doubt; in the three cases in which no precursor was found, and in the two more or less doubtful cases, the curves were not smooth, that is, the "start" was not quite sudden.

In Zi-ka-wei, during the fourteen months considered, the eighteen disturbances which began with quite a sudden start all had one or two preliminaries. The interval between the forerunner and the disturbance ranges from two to seventeen hours of perfectly smooth trace.

Zi-ka-wei, China, March 11.

J. DE MOIDREY.

Centre of Gravity of Annual Rainfall.

THE mere *a priori* criticism which Mr. Watt, in his letter to NATURE of April 14, has bestowed on my letter to NATURE of March 31, dealing with a large class of *concrete physical facts*, is, to my mind, far from satisfactory or sufficient. In the last sentence of my letter I anticipated that it might meet with some such "simple" algebraic criticism.

I did not, as is implied, assert that the use of the C.G. of the year's rainfall would dispense with the ordinary graphic representation of the monthly distribution, but that it appeared to be a convenient method for *comparing* the rainfall distribution at a number of stations in the same country for the same year, as well as at a single station, or for the mean rainfall of the same country, for a succession of years. Obviously, therefore, Mr. Watt's simple types of rainfall for his imaginary stations A, B, C, which belong to very different regions on the earth's surface, have no application, even in theory, to my "suggestions."

As for the practical character of my proposals, the following may be given. They are quite as useful as the comparison of the variation of rainfall with that of population for a decennium in India, which was the subject of a paper given some years ago in the Journal of the Royal Meteorological Society, London. A comparison of the variations in the C.G. of the mean monthly rainfall for the past 50 or 100 years with the agricultural results of those years in the British Isles, &c., for which the data may be available; an examination of these variations in connection with the much-discussed question of weather cycles, so commonly based on rainfall statistics; the detection of serious clerical slips in the tabulation of rainfall; the interpolation of the probable rainfall figures for a month in the event of a rain-gauge or measure-glass being temporarily unserviceable; the detection of the ignorant or inadvertent use of a wrong measure-glass—a matter of frequent occurrence in India, and possibly not unknown in this country.

Rain falls with such seeming irregularity of quantity and date, even in India—and much more so in this "unspeakable" Scotland—that it would appear *prima facie* impossible that there could be any approach to constancy of the date around which the whole year's rainfall balances. If we look at the tables of monthly rainfall for a large number of stations (in the same country) and see that the figures are not even approximately the same for the months or for the whole year, if we consider the difficulties connected with the measurement of rainfall, which are discussed in many of the volumes of "British Rainfall," it is surely surprising that, in spite of all these things, the reported year's rainfall should balance round a date which does not differ by more than a few days for a great many, if not all, the stations in any one year, and that for another year the displacement of this central date is so nearly the same for all of them. By merely looking at the monthly figures or at the graphs of those figures, we cannot accurately estimate either the central date or the amount of its displacement.

The causes which determine the times and amounts of rainfall for any place or country are known only in a very general way indeed. They are so elusive that investigators in their despair have even had recourse to sun-spots or comets' tails as a possible cause of special excess or deficiency of rainfall. They are so elusive that even in India, with its comparatively regular rainfall-seasons and with its special equipment of experts, the problem of

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correctly forecasting the monsoon rains either as to time or intensity is still far from complete solution. They are so elusive that the forecasting of rainfall even for three days in this part of the world is not yet conspicuous for its infallibility.

The importance of the laws of rainfall is so incalculably great that it is not extravagant to say that their discovery is the ultimate object of, and excuse for, the millions of meteorological figures that are published annually in all civilised nations. Hence the discussion of rainfall in all its aspects is worthy of encouragement, and in this connection the centre of gravity of annual rainfall may possibly, after all, be of some value. J. COOK.

Edinburgh, April 18.

I AM glad to read Mr. Cook's reply to my remarks, but believe that my criticism cannot be dismissed as a mere *a priori* one, and that it goes to the root of the matter. It is true that Mr. Cook illustrated his proposal in a most exhaustive manner, and that he did not suggest that his method might be of service in comparing the rainfalls of places in quite different climatic regions. But the general reasoning in the first paragraph of my former letter cannot be both correct and incorrect. Assuming it to be correct, it follows directly that even if we confine our attention to the records for a single station we might have the same C.G. for two years which differed greatly from one another as regards the monthly distribution of rainfall. In such a case, what possible significance could attach to the position of the C.G.?

I am heartily in sympathy with Mr. Cook's feeling that the discussion of rainfall in all its aspects is worthy of attention, but note that he himself does not maintain that his method is, but only that it may possibly be, of some value. It is certainly at first sight surprising that the calculated C.G. of rainfall for a large number of places for a given year in, say, Scotland should be very nearly the same, for the monthly rainfalls as ordinarily tabulated exhibit a bewildering complexity; but if the monthly values for the various stations are expressed as percentages of the year's total, the resulting picture is usually of a very simple and symmetrical character, which would lead one to anticipate that the C.G.s for the various stations would approximate closely.

ANDREW WATT.

Scottish Meteorological Society, 122 George Street, Edinburgh, April 22.

The Fertilising Influence of Sunlight.

THE beneficial effect of heat on soil is recorded by Virgil in the following passage, to which Mr. F. B. Smith has directed my attention :—

> "Often too, 'tis good
> To burn the stubbles and with crackling flames
> Consume the empty stalks; whether from thence
> The earth derives a hidden store of strength
> And fattening food, or whether 'tis that fire
> Rakes out the subtlest vice and sweats away
> Excessive damp, or whether by the heat
> New pores are opened and the choked are cleared," &c.
> ("Georgics," Bk. i., lines 100 *et seq.*).

It is interesting to learn from Mr. Fletcher (April 7) that the natives of Bombay, in certain circumstances, subject their soils to heat. Mr. Fletcher regards the explanation given by Dr. Hutchinson and myself as incorrect, and suggests that the effect is due to the destruction of some toxin. This was the first hypothesis we examined, but was found to be insufficient.

(1) Toluened soil (*i.e.* soil treated with a small quantity of toluene, which is subsequently allowed to evaporate *in situ* without washing anything from the soil) is more fertile and more favourable to bacterial activity than the original untreated soil.

(2) When an aqueous extract of untreated soil is added to the toluened soil, there is a still further increase in fertility and in bacterial activity. The same result follows when a minute amount of the untreated soil itself is added instead of the aqueous extract.

(3) When a larger quantity (5 per cent.) of the untreated soil is added a similar effect is produced for a time, then the bacterial activity begins to be depressed. This action increases, and finally the depression, both in bacterial activity and in fertility, is out of all proportion to the 5 per cent. of soil originally added.

Experiment (2) is conclusive against the hypothesis that a soluble toxin exists in the untreated soil which can be put out of action by toluene. For such a toxin should cause a decrease, and not an increase, in productiveness. Experiment (3) is equally conclusive against a relatively insoluble toxin; had this been present the depression should have shown itself at once, and should have been proportional to the amount of toxin, *i.e.* of untreated soil, added.

The *growth* of the injurious factor in experiment (3) seems to necessitate a biological hypothesis. Considering these and our other experiments in detail, Dr. Hutchinson and I see no way out of the conclusion that organisms are present in soil inhibiting the development of bacteria, and therefore of plant food. The organisms, whatever they are, must be larger than bacteria, or they would occur in the extract of experiment (2) along with the numerous bacteria there present—indeed, the beneficial effect of this extract was traced to the unweakened races of bacteria present, partial sterilisation having somewhat weakened the soil bacteria. Further, they develop more slowly than bacteria. As similar phenomena have been observed in all the soils examined, we are justified in supposing that the organisms are widely distributed, and constitute an important factor in soil fertility.

Mr. Fletcher's water-culture experiment is not germane to the point. A toxic body that occurred there would not necessarily come direct from the plant or be found in the soil. It is extraordinarily difficult to keep prolonged water cultures sterile, and until some attention is paid to the bacterial changes going on it is impossible to regard the results as proof of the presence of toxins in soils. Indeed, I know of no satisfactory evidence of their existence in normal soils. E. J. RUSSELL.

Rothamsted Experiment Station, Harpenden.

Pneumatolysis.

IT is thirty-nine years this month since NATURE, over diffident initials, published my first scientific communication that ever saw print. For more than thirty of those years I have been much interested in the physics of plutonic rocks. Quite recently an event has occurred which must be almost without precedent in science. The petrologists have apparently repudiated, with unanimity, what is an axiom beyond dispute with chemists.

For some years past the petrology of plutonic rocks has been based on the new doctrine of "pneumatolysis," or the solvent powers of gases over solids.

Perhaps the last published important work on chemistry is the English version of Ostwald's "Fundamental Principles of Chemistry," 1909. Referring in that work to a certain diagram, representing the behaviour of one solid and one gas, the author writes :—

"From this point the liquid phase exists in the presence of the gaseous phase to the end of the diagram, *because solid substances do not form solutions with gases*" (pp. 186–7). Italics mine.

I believe that every chemist will assent to the above statement. If a gas is to mix with a solid, as a solution, the solid must first be vapourised; but if this be so the greater part of twentieth-century petrology breaks down, because it is everywhere relying on the truth of pneumatolysis.

From the student's point of view the situation is as paralysing as it is stupefying, and there seems nothing to be done but to put away the microscope. It is no work for students to discuss first principles.

Southwood, Torquay, April 18. ARTHUR R. HUNT.

Anomalous Reading of Hygrometer.

MAY not the observation referred to in NATURE of April 7 (p. 165) be a very simple case of latent heat evolution by condensation when the atmosphere is supersaturated with vapour? I think I have seen the wet bulb registering a temperature higher than the dry bulb; but this explanation seemed at the time so obvious that I made no careful verification of the apparent phenomenon.

HUGH RICHARDSON.

Bootham School, York, April 11.

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